such as bleaching. While in agricultural districts much might be done towards making the field labourer a more intelligent being than he is usually supposed to be, by giving lessons in agricultural chemistry, the art of draining, the laws of evaporation and condensation, the value of woodland, the construction of various implements used, &c. But in every locality lessons should be given in physiology, health, and ventilation, while natural history would everywhere form a not less pleasing than instructive alternative subject. The walls of every school, upper, middle, or elementary, should be covered with diagrams, and at the foot of these should be descriptive and explanatory notes.

As regards the teachers themselves, it may be left an open question as to whether they should be required to go through the ordeal of an examination in the subjects they take up, or whether it may be taken for granted that the mental discipline undergone for two years in the training college is sufficient to enable them to perfect themselves for the task. Those among them who have been at work for ten or fifteen years might probably feel indisposed to prepare again for examination; and we are ready to admit that if a man can bring up his pupils to the required standard, he may well be excused from any other test. At any rate it is to be hoped that the subject of scientific instruction in our elementary schools will receive that attention from Government which is due to it, and that having been once again taken up, it will never be allowed to fall into desuetude.

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## The Prevalence of West Winds

IN NATURE of 29th September, 1870, there is an abstract of a paper read at the British Association by Mr. Laughton, maintaining that the preponderance of west over east winds over the entire globe is such as to point to a cosmical force of some kind, moving the atmosphere round the earth from west to east; and concluding "that the motive force for which we are seeking, is really the disturbing force of the attraction of the heavenly bodies."

The course here suggested, if it were operative at all, would act in the opposite direction to that required. The only possible effect of the attraction of the heavenly bodies on the atmosphere is an atmospheric tide, and the motion of a tide is necessarily from east to west, with the apparent diurnal motion of the heavens. But it is only when resisted that a tidal wave can give origin to currents, and it is scarcely probable that any tidal current can be formed in the atmosphere of sufficient force to blow a feather. I maintain the truth of the ordinary theory, that all winds originate in a disturbance of atmospheric equilibrium by unequal solar heating in different parts of the earth, but are changed in direction by the earth's rotation. This theory is quite consistent with the fact on which Mr. Laughton insists, that west winds preponderate over east winds. It is true that the mechanical effect of any wind whatever must be balanced by an equal mechanical effect in the opposite direction. Mr. Laughton appears to see that this would necessarily be the case if the ordinary theory of the winds is true, but he does not see how the compensation is effected. Were the winds all from the west, they would in an infinitesimal degree accelerate the earth's rotation; were they all from the east, they would in an infinitesimal degree retard it: and if either of these effects were proved to take place, the inference would be certain that the winds are set in motion by some other agency than unequal heating in different places; because, in virtue of the universally true principle of the "conservation of rotation," no such motive power could have any effect on the earth's rotation whatever.

The unbalanced effect of any wind on the earth's rotation will be due to the product of three factors, namely, the area covered by the wind, the east or west component of its force, and the radius of the parallel of latitude at the place. Mr. Laughton has probably overlooked the last-mentioned factor. It gives *leverage*. An east wind near the equator has more effect in retarding the rotation of the earth than a west wind of equal extent and force at a higher latitude has in accelerating it, just as a weight at the end of the long arm of a lever outweighs an equal weight at the end of the short arm. It is for this reason that the west winds, which are mostly in the higher latitudes, are of greater force, and probably cover a greater area than the east winds, which, under the name of the trade-winds, predominate near the equator.

JOSEPH JOHN MURPHY
Old Forge, Dunmurry, Co. Antrim, Jan. 25

## Can Weather be Influenced by Artificial Means?

SOME remarks on this subject made by the Rev. R. B. Belcher before the Geological Section of the British Association (reported in the *Athenæum* of October 29), reopen a question of popular meteorology, which has not, perhaps, been sufficiently attended to, from an exact and scientific point of view. Such evidence on the subject, as is at present available to the public, is too general to have much claim to correctness of detail on points which require particular and *local* information; and I offer the following abstract of it principally in the hope that it may lead to further inquiry and observation.

The idea that large fires do, in some way, bring on rain, is very old; but it was, I believe, for the first time stated as a fact and explained on scientific grounds by the late Professor Espy. His theory is, that the heating of the air causes a rapidly ascending current, and that the moisture which air near the surface always contains, is thus carried into the upper regions of the atmosphere to be condensed and to fall as rain. In support of this view, he has given several instances in which rain did immediately follow the kindling of a fire, when no clouds had previously been visible, but in a problem of this nature, negative examples have more weight than positive; and it is necessary to admit that, though in some very remarkable instances rain has followed a large fire, in other instances, quite as remarkable, there is no notice of rain. It is, of course, difficult to speak with absolute certainty; it may almost always be said that a few drops have fallen, sufficient to bear out the truth of Espy's theory, but it seems to me that to establish it in its entirety, something more than this is necessary, and that an extraordinary fire ought to produce a very decided shower, if not a heavy downpour. Last summer, in July, a large fire raged for several days on the moorland and through the pine copses a few miles to the east of Wimborne-Minster. The weather was hot and dry. I was myself on a walking tour in that part of the country, and passed close by this fire, near the village of Longham, where it had been burning with great fury for a week. The farmers were all complaining of the long and excessive drought, and I can give my personal testimony to the fact that there was absolutely no rain. Similarly in the very dry summer of '68, the papers were for some weeks filled with accounts of fires raging fiercely in various parts of Wales and Scotland; but there was no word of any break in the drought. It is only by referring to exceptional and recent examples of this kind that we can feel any degree of certainty; wet weather and heavy rain are so common in this country, that under circumstances not very remarkable, they would scarcely be noticed; and these instances I have just mentioned show the necessity for caution before attributing to fire and consequent rain the relationship of cause and effect; and, as bearing directly on Espy's theory, I may add that there are no observations which would show that the rainfall in our large towns is greater than in the adjacent country; yet the thousands of fires in London, for example, must heat the air sufficiently to cause some ascensional tendency.

The evidence regarding the effect of violent explosions is in very much the same state of imperfection. Theoretically, we can admit that a violent shock may throw large masses of the lower air to a considerable height, so as to cause condensation of its vapour; and there are many instances which seem to support the conclusion that it does do so. The clearest on record are perhaps those mentioned by Dr. Darwin, in his Journal of Researches, where he describes how on several occasions a shower of rain fell, in the middle of the *dry* season, in Chili and other parts of South America, after an earthquake. After the earthquake in Peru, in the latter part of the year 1868, I went carefully through the published accounts, but found no mention of any similar showers. In our own climate, in which rain is not uncommon at any time of the year, the circumstances require to be examined into more critically, and the evidence afforded by them can never be quite so satisfactory. Whitaker's Almanack for last year, in the list of Remarkable Occurrences, &c., for 1869, mentions several serious explosions of powder mills, collieries, &c., but not one of them is followed by any notice of remarkable weather, storm, or rain; still, as one of these is the conflagration at Bordeaux on September 28th, which, as Mr. Belcher has pointed out, was followed by very heavy gales, both in the Bay of Biscay and in the English Channel, it is likely enough that heavy weather following some of the other accidents may have been omitted, or have been insufficient to call for special mention in such a general list.

The effect of great battles, again, has always been a favourite

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theme amongst popular meteorologists; but in regard to this also there is the same uncertainty. Several battles and bombardments have been followed by storms; but, on the other hand, several have not. The general opinion amongst naval men is that heavy firing beats down the wind and produces a calm; and it is tolerably certain that this is frequently the case, more especially if the wind is light. Whether it does or not, the sudden irruption of millions of cubic feet of gas into the lowest strata of the atmosphere and within a very limited area, must have a tendency to cause disturbance, a tendency increased by the undulatory movement due to the noise of the guns. The battle of Trafalgar (October 21st) was fought in a very light westerly breeze, which, towards the afternoon, almost entirely died away. During the night the wind gradually freshened, and by noon the next day blew very hard. Lord Collingwood in his despatch, dated the 22nd, says it had "blown a gale of wind ever since the action;" but that this is merely a general way of speaking is seen by a reference to the logs of the various ships of the fleet. (Despatches of Lord Nelson, vol. vii., pp. 159, *et seq.*) A westerly gale, on the coast of Portugal or Spain, is not such an unusual occurrence in the end of October, as to compel us to attribute one to a battle which took place twenty hours before it came on. Again, the battle of St. Vincent, of the Nile, Rodney's action off St. Domingo, and many others, do not seem to have been followed by any interruption to the usual fine weather. Quatre-bras was followed by heavy rain; but the night after Waterloo was fine, and the bright moonlight is specially mentioned as advantageous to the pursuers; and though Mr. Belcher speaks of rain as having set in in the east of France during the present campaign, I have not seen any notice of its following the battles of Gravelotte or Sedan, so as to be clearly referable to them.

What appears to me the only explanation of the apparent contradictions in the evidence is this; that large fires, explosions, battles, and earthquakes, do tend to cause atmospheric disturbance, and especially, to induce a fall of rain; but that for the tendency to produce effect, it is necessary that other conditions should be suitable; that rain does not follow, unless the lower air contain a great deal of moisture; and that therefore the ascensional movement does not reach to a height such as we might at first be led to conceive; that, in fact, the height is for the most part very trifling. With regard to storms said to have been caused by some of these agencies, the evidence is still more unsatisfactory; and, in our present ignorance of the cause of storms generally, is quite insufficient to compel us to attribute any one particular gale, extending probably over a wide area, to some very limited and comparatively insignificant disturbance.

J. K. Laughton

## Natural Science at Cambridge

The rejoinder in your last number (p. 287) to my inquiries is rather ingenious than ingenuous. "The Writer of the Article in Question" vouchsafes no answer to my very plain questions (p. 264), but is kind enough, instead of justifying his own statement, to propound sundry suggestions, surmises, and what not. I simply asked for confirmation of a certain assertion. This he avoids giving, and, though I regret the cause, I do not wonder at it. Some persons may think it would have been better taste to have acknowledged that the assertion was unfounded. Perhaps your correspondent is of another opinion; if so, I do not wish to interfere with him. I have only to remark that substantially he admits the force of the doubt I expressed by now modifying his previous statement as to "most of the colleges" into "some colleges"—perhaps "one college" would have been nearer the mark, but I will let that pass, hoping that when he again writes on the subject, events may have proved him to be a true prophet.

Cambridge, Feb. 10 M. A.

## Glass Globes

In consequence of the letter of Col. Greenwood, which appeared in Nature of the 1st December last (No. 57, p. 87), I wrote to Dr. G. O. Sars, of Christiania, who is the Inspector of Sea-fisheries in Norway, for information on the subject. In his reply, just received, he says he was not aware that such globes were used as net floats in any other country than Norway, and that until quite recently they had been used there in the great cod fisheries at Lofoten only. At Söndmör, in Christiansand, they had begun to come into use in 1869, but by no means generally He has no doubt that the floats washed ashore in Shetland and the Hebrides came from Lofoten; but he remarks that their course in a south-westerly direction is quite contrary to what might be expected from the action of the equatorial current; and he suggests that before reaching our northern coasts the floats made a very long course, having been first carried northwards until they got within the range of the Polar stream, by which they may have been carried southwards along the coast of Greenland, and so at last coming within the influence of the Gulf-stream, they were carried eastward across the Atlantic, and thence drifted by south-westerly winds to their destination. He adds that the fact of their having been found on the *west* coast of the Isle of Lewis seems to confirm this hypothesis.

Feb. 13 J. Gwyn Jeffreys

P.S. on another subject. The correspondents in recent numbers of Nature do not appear to have been aware of Dr. Carpenter's and my remarks in the *Athenæum* of the 22nd of October last on the hypothesis of the Cretaceous formation being continued or not continued up the present time.

## The Primary Colours

Here is another proof that violet is the third primary. Prof. Tyndall, with that sagacity which results from the right use of his imagination, has given us a reason for the blue colour of the sky. It is probably the true reason, and it proves that blue is not a primary colour. The smallest light-waves are those of the extreme violet. These, therefore, are the waves which will be reflected in greatest proportion by the minute particles of foreign matter in the air. Why is not the sky violet then, instead of blue? Plainly, because although violet predominates in the reflected light, there is also a small proportion of green and a still smaller proportion of red. Suppose the ratios to be 1 red, 3 green, 6 violet, and suppose the ratios for white light to be 1 red, 2 green, 4 violet. Then, the light of the sky will be white *plus* 1 green, 2 violet, which is blue. In Professor Tyndall's experiment on the decomposition of sulphurous acid gas by a beam from the electric lamp, he tells us that, as the particles of the sulphur cloud grow larger, the colour changes from pale blue to deeper blue, and then to whitish blue and white.

No colour but blue makes its appearance, because there is always some green and red, as well as violet; and always a surplus of green *over* red, more than enough for white light.

Leicester, Jan. 28 Frederick T. Mott

## Yellow

It would seem to me that the great difficulty of conceiving yellow as a compound colour is the brightness or lightness of yellow, as compared with its components. In the spectrum, we have the maximum of light in the yellow, and it is against our experience to put two dark colours together and form one light one, as, for example, to put the red and green together and form yellow. But there is just the same difficulty in the production of white light from all the colours of the spectrum, for we regard white as lighter than any of the colours, even than yellow. This, however, is a mental fallacy, and if once exposed, seems to me to do away with the difficulty of conceiving white as made up of a series of colours. Our ideas of colours and tints are derived from our own experience, and we produce tints by precisely the opposite method to that of nature. Nature's method of painting a blade of grass is, to throw on it all the colours of the spectrum, and afterwards pick out those which shall have a *residue* of green. The artist's method is to see what colours will produce green and then lay those on. I do not think it too much to say that all colours are merely tints of white—that, for example, yellow is really yellowish white, green, greenish white, and so on. If we take two cards, one green, the other red, and hold them so that the light is properly reflected from the one on to the other, the cards do not appear black but white. If the red and green were pure colours, the cards should of course appear black, for the light from the green card would be totally absorbed by the red card, and similarly with the red card. The white cannot be produced by a combination of the red and green, but by the extinction on one card of the green that gives the greenish white, and on the other of the red which gives the reddish white. That yellow is but little different from white is well illustrated in the beautiful experiment of Newton's of syn-

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